XIII. *Investigation of the Powers of the prismatic Colours to heat and illuminate Objects; with Remarks, that prove the different Refrangibility of radiant Heat. To which is added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers.*

By William Herschel, *LL. D. F. R. S.*

Read March 27, 1800.

It is sometimes of great use in natural philosophy, to doubt of things that are commonly taken for granted; especially as the means of resolving any doubt, when once it is entertained, are often within our reach. We may therefore say, that any experiment which leads us to investigate the truth of what was before admitted upon trust, may become of great utility to natural knowledge. Thus, for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose, that every one of the united rays contributes its proportional share to the intensity of the heat which is produced; and we should probably think it highly absurd, if it were asserted that many of them had but little concern in the combustion, or vitrification, which follows, when an object is put into that focus. It will therefore not be amiss to mention what gave rise to a surmise, that the power of heating and illuminating objects, might not be equally distributed among the variously coloured rays.

In a variety of experiments I have occasionally made, relating

to the method of viewing the sun, with large telescopes, to the best advantage, I used various combinations of differently-coloured darkening glasses. What appeared remarkable was, that when I used some of them, I felt a sensation of heat, though I had but little light; while others gave me much light, with scarce any sensation of heat. Now, as in these different combinations the sun's image was also differently coloured, it occurred to me, that the prismatic rays might have the power of heating bodies very unequally distributed among them; and, as I judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision, by possessing a superior illuminating power. At all events, it would be proper to recur to experiments for a decision.

Experiments on the heating Power of coloured Rays.

I fixed a piece of pasteboard, AB, (Plate X.) in a frame, mounted upon a stand, CD, and moveable upon two centres. In the pasteboard, I cut an opening, *mn*, a little larger than the ball of a thermometer, and of a sufficient length to let the whole extent of one of the prismatic colours pass through. I then placed three thermometers upon small inclined planes, EF: their balls were blacked with japan ink. That of No. 1 was rather too large for great sensibility. No. 2 and 3 were two excellent thermometers, which my highly esteemed friend Dr. WILSON, late Professor of Astronomy at Glasgow, had lent me for the purpose: their balls being very small, made them of exquisite

sensibility. The scales of all were properly disengaged from the balls.

I now placed the stand, with the framed pasteboard and the thermometers, upon a small plain board, GH; that I might be at liberty to move the whole apparatus together, without deranging the relative situation of the different parts.

This being done, I set a prism, moveable on its axis, into the upper part of an open window, at right angles to the solar ray, and turned it about till its refracted coloured spectrum became stationary, upon a table placed at a proper distance from the window.

The board containing the apparatus was now put on the table, and set in such a manner as to let the rays of one colour pass through the opening in the pasteboard. The moveable frame was then adjusted to be perpendicular to the rays coming from the prism; and the inclined planes carrying the three thermometers, with their balls arranged in a line, were set so near the opening, that any one of them might easily be advanced far enough to receive the irradiation of the colour which passed through the opening, while the rest remained close by, under the shade of the pasteboard.

By repeated trials, I found that Dr. WILSON's No. 2 and mine, always agreed in shewing the temperature of the place where I examined them, when the change was not very sudden; but that mine would require ten minutes to take a change, which the other would shew in five. No. 3 never differed much from No. 2.

1*st Experiment.* Having arranged the three thermometers in the place prepared for the experiment, I waited till they were stationary. Then, advancing No. 1 to the red rays, and leaving

the other two close by, in the shade, I marked down what they shewed, at different times.

No. 1.		No. 2.		No. 3.
$43\frac{1}{2}$	- - -	$43\frac{1}{2}$	- - -	$43\frac{1}{4}$
48	- - -	$43\frac{1}{2}$	- - -	$43\frac{1}{2}$
$49\frac{1}{2}$	- - -	$43\frac{1}{4}$	- - -	$43\frac{1}{4}$
$49\frac{3}{4}$	- - -	$43\frac{1}{4}$	- - -	$43\frac{1}{4}$
50	- - -	$43\frac{1}{4}$	- - -	$43\frac{1}{4}$

This, in about 8 or 10 minutes, gave $6\frac{3}{4}$ degrees, for the rising produced in my thermometer, by the red rays, compared to the two standard thermometers.

2d *Experiment*. As soon as my thermometer was restored to the temperature of the room, which I hastened, by applying it to a large piece of metal that had been kept in the same place, I exposed it again to the red rays, and registered its march, along with No. 2 as a standard, which was as follows.

No. 1.		No. 2.
45	- - -	45
48	- - -	45
51	- - -	45
51	- - -	$44\frac{1}{2}$
51	- - -	44

Hence, in 10 minutes, the red rays made the thermometer rise 7 degrees.

3d *Experiment*. Proceeding in the same manner as before, in the green rays I had,

No. 1.				No. 2.
43	-	-	-	43
$45\frac{1}{2}$	-	-	-	43
46	-	-	-	43
46	-	-	-	$42\frac{3}{4}$
46	-	-	-	$42\frac{3}{4}$

Therefore, in ten minutes, the green rays occasioned a rise of $3\frac{1}{4}$ degrees.

4th Experiment. I now exposed my thermometer to the violet rays, and compared it with No. 2.

No. 1.				No. 2.
44	-	-	-	44
44	-	-	-	44
$44\frac{3}{4}$	-	-	-	$43\frac{1}{2}$
45	-	-	-	43

Here we have a rising of 2 degrees, in ten minutes, for the violet rays.

5th Experiment. I now exposed Dr. Wilson's thermometer No. 2 to the red rays, and compared its progress with No. 3.

No. 2.				No. 3.
44	-	-	-	44
46	-	-	-	44
$46\frac{1}{2}$	-	-	-	$43\frac{3}{4}$
$46\frac{1}{2}$	-	-	-	$43\frac{3}{4}$

Here the thermometer, exposed to red, rose in five minutes $2\frac{3}{4}$ degrees.

6th Experiment. In red rays again.

No. 2.				No. 3.
44	-	-	-	44
46	-	-	-	44
$46\frac{1}{2}$	-	-	-	$43\frac{1}{2}$
47	-	-	-	$43\frac{1}{2}$
47	-	-	-	43

And here the thermometer, exposed to red, rose in five minutes 4 degrees.

7th Experiment. In green rays.

No. 2.				No. 3.
$43\frac{1}{2}$	-	-	-	$43\frac{1}{2}$
$44\frac{1}{2}$	-	-	-	$43\frac{1}{2}$
$44\frac{1}{2}$	-	-	-	43

This made the thermometer rise, in the green rays, $1\frac{1}{2}$ degree.

8th Experiment. Again in green rays.

No. 2.				No. 3.
43	-	-	-	43
$44\frac{1}{2}$	-	-	-	$42\frac{3}{4}$
$44\frac{3}{4}$	-	-	-	$42\frac{3}{4}$

Here the rising, by the green rays, was 2 degrees.

From these experiments, we are authorised to draw the following results. In the red rays, my thermometer gave $6\frac{3}{4}$ degrees in the 1st, and 7 degrees in the 2d, for the rising of the quicksilver: a mean of both is $6\frac{7}{8}$. In the 3d experiment, we had $3\frac{1}{4}$ degrees, for the rising occasioned by the green rays; from which we obtain the proportion of 55 to 26, for the power of heating in red to that in green. The 4th experiment gave

2 degrees for the violet rays; and therefore we have the rising of the quicksilver in red to that in violet, as 55 to 16.

A sufficient proof of the accuracy of this determination we have, in the result of the four last experiments. The rising for red rays in the 5th, is $2\frac{3}{4}$; and in the 6th, 4 degrees: a mean of both is $3\frac{3}{8}$. In the 7th experiment, we have $1\frac{1}{2}$, and in the 8th, 2 degrees, for the rising in green: a mean of these is $1\frac{3}{4}$. Therefore, we have the proportion of the rising in red to that in green, as 27 to 11, or as 55 to 22,4.

We may take a mean of the result of both thermometers, which will be 55 to 24,2, or more than $2\frac{1}{4}$ to 1, in red to green; and about $3\frac{1}{2}$ to 1, in red to violet.

It appears remarkable, that the most sensible thermometer should give the least alteration, from the exposure to the coloured rays. But since, in these circumstances, there are two causes constantly acting different ways; the one to raise the thermometer, the other to bring it down to the temperature of the room, I suppose, that on account of the smallness of the ball in Dr. WILSON's No. 1, which is but little more than $\frac{1}{8}$ of an inch, the cooling causes must have a stronger effect on the mercury it contains than they can have on mine, the ball of which is half an inch.

More accuracy may hereafter be obtained, by attending to the circumstances of blacking the balls of the thermometers, and their exposure to a more steady and powerful light of the sun, at greater altitudes than it can be had at present; but the experiments which have been related, are quite sufficient for my present purpose; which only goes to prove, that the heating power of the prismatic colours, is very far from being

equally divided, and that the red rays are chiefly eminent in that respect.

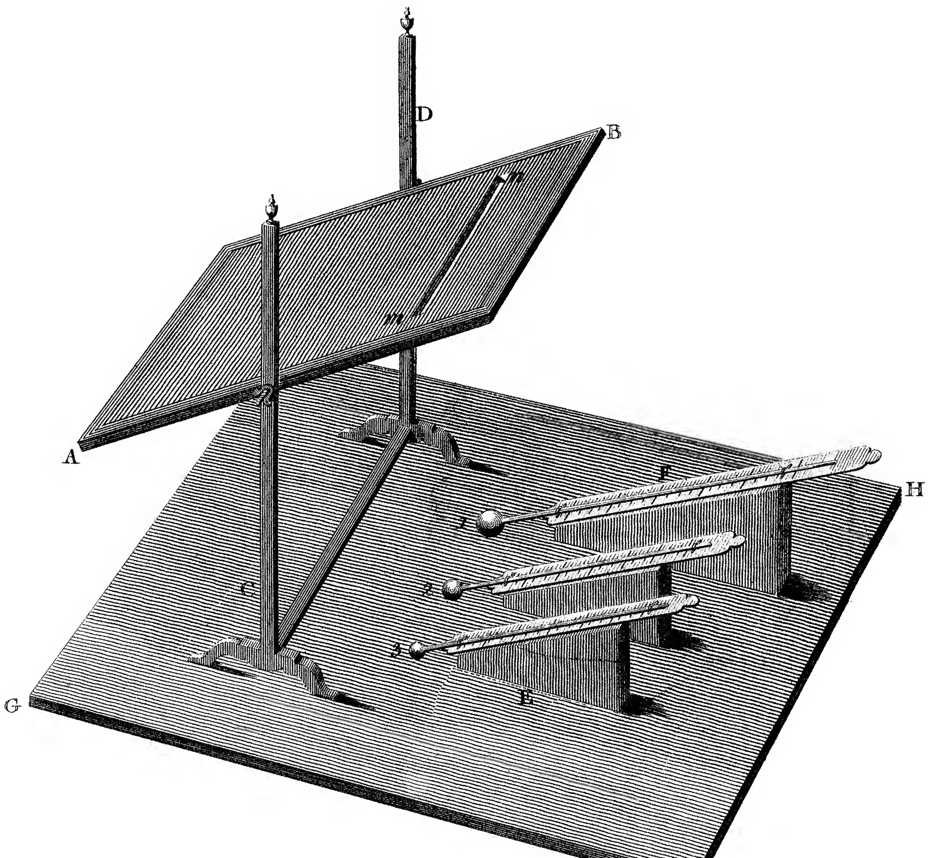
Experiments on the illuminating Power of coloured Rays.

In the following examination of the illuminating power of differently-coloured rays, I had two ends in view. The first was, with regard to the illumination itself; and the next, with respect to the aptness of the rays for giving distinct vision; and, though there did not seem to be any particular reason why these two should not go together, I judged it right to attend to both.

The microscope offered itself as the most convenient instrument for this investigation; and I thought it expedient to view only opaque objects, as these would give me an opportunity to use a direct prismatic ray, without running the risk of any bias that might be given to it, in its transmission through the colouring particles of transparent objects.

1*st Experiment*. I placed an object that had very minute parts, under a double microscope; and, having set a prism in the window, so as to make the coloured image of the sun stationary upon the table where the microscope was placed, I caused the differently-coloured rays to fall successively on the object, by advancing the microscope into their light. The magnifying power was 27 times.

In changing the illumination, by admitting a different colour, it always becomes necessary to re-adjust the instrument. It is well known, that the different refrangibility of the rays will



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sensibly affect the focal length of object-glasses; but, in compound vision, such as in a microscope, where a very small lens is made to cast a lengthened secondary focus, this difference becomes still more considerable.

By an attentive and repeated inspection, I found that my object was very well seen in red; better in orange, and still better in yellow; full as well in green; but to less advantage in blue; indifferently well in indigo, and with more imperfection in violet.

This trial was made upon one of the microscopic objects which are generally prepared for transparent vision; but, as I used it in the opaque way, I thought that others might be chosen which would answer the purpose better; and, in order to give some variety to my experiments, and to see the effect differently coloured substances might have on the rays of light, I provided the following materials to be viewed. Red paper; green paper; a piece of brass; a nail; a guinea; black paper. Having also found that a higher power might be used, with sufficient convenience for the rays of light to come from the prism to the object, I made the miscroscope magnify 42 times.

The appearance of the nail in the microscope, is so beautiful, that it deserves to be noticed; and the more so, as it is accompanied with circumstances that are very favourable for an investigation, such as that which is under our present consideration. I had chosen it on account of its solidity and blackness, as being most likely to give an impartial result, of the modifications arising from an illumination by differently-coloured rays; but, on viewing it, I was struck with the sight of a bright constellation of thousands of luminous points, scattered over its whole extent, as far as the field of the microscope could take it in.

Their light was that of the illuminating colour, but differed considerably in brightness; some of the points being dim and faint, while others were luminous and brilliant. The brightest of them also, admitted of a little variation in their colour, or rather in the intensity of the same colour; for, in the centre of some of the most brilliant of these lucid appearances, their light had more vivacity, and seemed to deviate from the illuminating tint towards whiteness, while on and near the circumference, it appeared to take a deeper hue.

An object so well divided by nature, into very minute and differently-arranged points, on which the attention might be fixed, in order to ascertain whether they would be equally distinct in all colours, and whether their number would be increased or diminished by different degrees of illumination, was exactly what I wanted; nor could I think it less remarkable, that all the other objects I had fixed upon, besides many more which have been examined, such as copper, tin, silver, *&c.* presented themselves nearly with the same appearance. In the brass, which had been turned in a lathe, the luminous points were arranged in furrows; and in tin they were remarkably beautiful. The result of the examination of my objects was as follows.

2d Experiment. Red paper.

In the red rays, I view a bright point near an accidental black spot in the paper, which serves me as a mark; and I notice the space between the point and the spot: it contains several faint points.

In the orange rays, I see better. The bright point, I now perceive, is double.

In the yellow rays, I see the object still better.

In the green rays, full as well as before.

In the blue rays, very well.

In the indigo rays, not quite so well as in the blue.

In the violet rays, very imperfectly.

3d Experiment. Green paper.

Red. I fix my attention on many faint points, in a space between two bright double points.

Orange. I see those faint points better.

Yellow. Still better.

Green. As well as before. I see remarkably well.

Blue. Less bright, but very distinct.

Indigo. Not well.

Violet. Bad.

4th Experiment. A piece of very clean turned brass.

R. I remark several faint luminous points between two bright ones. The colour of the brass makes the red rays appear like orange.

O. I see better, but the orange colour is likewise different from what it ought to be; however, this is not, at present, the object of my investigation.

Y. I see still better.

G. I see full as well as before.

B. I do not see so well now.

I. I cannot see well.

V. Bad.

5th Experiment. A nail.

R. I remark two bright points, and some faint ones.

O. Brighter than before; and more points visible. Very distinct.

Y. Much brighter than before; and more points and lines visible. Very distinct.

G. Full as bright; and as many points visible. Very distinct.

B. Much less bright. Very distinct.

I. Still less bright. Very distinct.

V. Much less bright again. Very distinct.

6th Experiment. I viewed a guinea, at 9 feet 6 inches from the prism; and adjusted the place of the object in the several rays, by the shadow of the guinea. If this be not done, deceptions will take place.

R. Four remarkable points. Very distinct.

O. Better illuminated. Very distinct.

Y. Still better illuminated. Very distinct. The points all over the field of view are coloured; some green; some red; some yellow; and some white, encircled with black about them.

Between yellow and green is the maximum of illumination. Extremely distinct.

G. As well illuminated as the yellow. Very distinct.

B. Much inferior in illumination. Very distinct.

I. Badly illuminated. Distinct.

V. Very badly illuminated. I can hardly see the object at all.

7th Experiment. The nail again, at 8 feet from the prism.

R. I attended to two bright points, with faint ones between them. Almost all the points in the field of view are red. Very distinct.

O. I see all the points better: they are red, green, yellow, and whitish, with black about them. Very distinct.

Y. I see better. More bright points, and more faint ones: the points are of various colours. Very distinct.

G. I see as well. The points are mostly green, and brightish-green, inclining to white. Very distinct.

B. Much worse illuminated. Very distinct.

I. Badly illuminated. Very distinct.

V. There is hardly any illumination.

8th Experiment. The nail again, at 9 feet 6 inches from the prism, by way of having the rays better separated.

R. Badly illuminated. The bright points are very distinct.

O. Much better illuminated. The bright points very distinct.

Y. Still better illuminated. All points extremely distinct.

G. As well illuminated, and equally distinct.

B. Badly illuminated. The bright points are distinct; but the others are not so.

I. Very badly illuminated. I do not see distinctly; but I believe it to be for want of light.

V. So badly illuminated that I cannot see the object; or at least but barely perceive that it exists.

9th Experiment. Black paper, at 8 feet from the prism.

R. The object is hardly visible. I can only see a few faint points.

O. I see several bright points, and many faint ones.

Y. Numberless bright and small faint points.

Between yellow and green, is the maximum of illumination.

G. The same as the yellow.

B. Very indifferently illuminated; but not so bad as in the red rays.

I. I cannot see the object.

V. Totally invisible.

From these observations, which agree uncommonly well, with respect to the illuminating power assigned to each colour, we may conclude, that the red-making rays are very far from having it in any eminent degree. The orange possess more of it than the red; and the yellow rays illuminate objects still more

perfectly. The maximum of illumination lies in the brightest yellow, or palest green. The green itself is nearly equally bright with the yellow; but, from the full deep green, the illuminating power decreases very sensibly. That of the blue is nearly upon a par with that of the red; the indigo has much less than the blue; and the violet is very deficient.

With regard to the principle of distinctness, there appears to be no deficiency in any one of the colours. In the violet rays, for instance, some of the experiments mention that I saw badly; but this is to be understood only with respect to the number of small objects that could be perceived; for, although I saw fewer of the points, those which remained visible were always as distinct as, in so feeble an illumination, could be expected. It must indeed be evident, that by removing the great obstacle to distinct vision, which is, the different refrangibility of the rays of light, a microscope will be capable of a much higher degree of distinctness than it can be under the usual circumstances. A celebrated optical writer has formerly remarked, that a fly, illuminated by red rays, appeared uncommonly distinct, and that all its minute parts might be seen in great perfection; and, from the experiments which have been related, it appears that every other colour is possessed of the same advantage.

I am well aware that the results I have drawn from the foregoing experiments, both with regard to the heating and illuminating powers of differently-coloured rays, must be affected by some little inaccuracies. The prism, under the circumstances in which I have used it, could not effect a complete separation of the colours, on account of the apparent diameter of the sun, and the considerable breadth of the prism itself, through which the rays were transmitted.

Perhaps an arrangement like that in Fig. 16, of the Newtonian experiments, might be employed; if instruments of sufficient sensibility, such as air thermometers, can be procured, that may be affected by the enfeebled illumination of rays that have undergone four transmissions, and eight refractions; and especially when their incipient quantity has been so greatly reduced, in their limited passage through a small hole at the first incidence.

But it appeared most expedient for me, at present, to neglect all further refinements, which may be attempted hereafter at leisure. It may even be presumed that, had there not been some small admixture of the red rays in the other colours, the result would have been still more decisive, with regard to the power of heating vested in the red rays. And it is likewise evident, that at least the red light of the prismatic spectrum, was much less adulterated than any of the other colours; their refractions tending all to throw them from the red. That the same rays which occasion the greatest heat, have not the power of illumination in any strong degree, stands on as good a foundation. For, since here also they have undergone the fairest trial, as being most free from other colours, it is equally proved that they illuminate objects but imperfectly. There is some probability that a ray, purified in the Newtonian manner above quoted, especially in a well darkened room, may remain bright enough to serve the purpose of microscopic illumination, in which case, more precision can easily be obtained.

The greatest cause for a mixture of colours, however, which is, the breadth of the prism, I saw might easily be removed; therefore, on account of the coloured points, which have been mentioned in the 6th and 7th experiments, I was willing to try

whether they proceeded from this mixture; and therefore covered the prism in front with a piece of pasteboard, having a slit in it of about $\frac{1}{10}$ of an inch broad.

10*th Experiment.* The nail, at 9 feet 2 inches from the prism.

R. I fix my attention on two shining, red points; they are pretty bright.

O. I see many more points. The object is better illuminated than in the red. The points are surrounded by black; but are orange-coloured.

Y. The points now are yellow, and white surrounded by black. The object is better illuminated than in orange.

The maximum of illumination is in the brightest yellow, or palest green.

G. The points are green and white, as before surrounded by black. Better illuminated than in orange.

B. The illumination is nearly equal to red.

I. Very indifferently illuminated.

V. Very badly illuminated.

The phænomena of the differently-coloured points being now completely resolved, since they were plainly owing to the former admixture of colours, and the illuminating power remaining ascertained as before, I attempted also to repeat the experiments upon the thermometer, with the prism covered in the same manner; but I found the effect of the coloured rays too much enfeebled to give a decisive result.

I might now proceed to my next subject; but it may be pardonable if I digress for a moment, and remark, that the foregoing researches ought to lead us on to others. May not the chemical properties of the prismatic colours be as different as those which relate to light and heat? Adequate methods for an investigation

of them may easily be found; and we cannot too minutely enter into an analysis of light, which is the most subtle of all the active principles that are concerned in the mechanism of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our daily observation, but which have hitherto remained unexplained. If the power of heating, as we now see, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the state of a red glow; and for the heat which is given by charcoal, coke, and balls of small-coal mixed up with clay, used in hot-houses; all which, it is well known, throw out red light. It also explains the reason why the yellow, green, blue, and purple flames of burning spirits mixed with salt, occasion so little heat that a hand is not materially injured, when passed through their coruscations. If the chemical properties of colours also, when ascertained, should be such that an acid principle, for instance, which has been ascribed to light in general, on account of its changing the complexion of various substances exposed to it, may reside only in one of the colours, while others may prove to be differently invested, it will follow, that bodies may be variously affected by light, according as they imbibe and retain, or transmit and reflect, the different colours of which it is composed.

Radiant Heat is of different Refrangibility.

I must now remark, that my foregoing experiments ascertain beyond a doubt, that radiant heat, as well as light, whether they be the same or different agents, is not only refrangible,

but is also subject to the laws of the dispersion arising from its different refrangibility; and, as this subject is new, I may be permitted to dwell a few moments upon it. The prism refracts radiant heat, so as to separate that which is less efficacious, from that which is more so. The whole quantity of radiant heat contained in a sun-beam, if this different refrangibility did not exist, must inevitably fall uniformly on a space equal to the area of the prism; and, if radiant heat were not refrangible at all, it would fall upon an equal space, in the place where the shadow of the prism, when covered, may be seen. But, neither of these events taking place, it is evident that radiant heat is subject to the laws of refraction, and also to those of the different refrangibility of light. May not this lead us to surmise, that radiant heat consists of particles of light of a certain range of momenta, and which range may extend a little farther, on each side of refrangibility, than that of light? We have shewn, that in a gradual exposure of the thermometer to the rays of the prismatic spectrum, beginning from the violet, we come to the maximum of light, long before we come to that of heat, which lies at the other extreme. By several experiments, which time will not allow me now to report, it appears that the maximum of illumination has little more than half the heat of the full red rays; and, from other experiments, I likewise conclude, that the full red falls still short of the maximum of heat; which perhaps lies even a little beyond visible refraction. In this case, radiant heat will at least partly, if not chiefly, consist, if I may be permitted the expression, of invisible light; that is to say, of rays coming from the sun, that have such a momentum as to be unfit for vision. And, admitting, as is highly probable, that the organs of sight are only adapted to receive impressions from

particles of a certain momentum, it explains why the maximum of illumination should be in the middle of the refrangible rays; as those which have greater or less momenta, are likely to become equally unfit for impressions of sight. Whereas, in radiant heat, there may be no such limitation to the momentum of its particles. From the powerful effects of a burning lens, however, we gather the information, that the momentum of terrestrial radiant heat is not likely to exceed that of the sun; and that, consequently, the refrangibility of *calorific* rays cannot extend much beyond that of *colourific* light. Hence we may also infer, that the invisible heat of red-hot iron, gradually cooled till it ceases to shine, has the momentum of the invisible rays which, in the solar spectrum viewed by day-light, go to the confines of red; and this will afford an easy solution of the reflection of invisible heat by concave mirrors.

Application of the Result of the foregoing Observations, to the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers.

Some time before the late transit of Mercury over the disk of the sun, I prepared my 7-feet telescope, in order to see it to the best advantage. As I wished to keep the whole aperture of the mirror open, I soon cracked every one of the darkening slips of wedged glasses, which are generally used with achromatic telescopes: none of them could withstand the accumulated heat in the focus of pencils, where these glasses are generally placed. Being thus left without resource, I made use of red glasses; but was by no means satisfied with their performance.

My not being better prepared, as it happened, was of no consequence; the weather proving totally unfavourable for viewing the sun at the time of the transit. However, as I was fully aware of the necessity of providing an apparatus for this purpose, since no method that was in use could be applied to my telescopes, I took the first opportunity of beginning my trials.

The instrument I wished to adapt for solar inspection, was a NEWTONIAN reflector, with 9 inches aperture; and my aim was, to use the whole of it open.

I began with a red glass; and, not finding it to stop light enough, took two of them together. These intercepted full as much light as was necessary; but I soon found that the eye could not bear the irritation, from a sensation of heat, which it appeared these glasses did not stop.

I now took two green glasses; but found that they did not intercept light enough. I therefore smoked one of them; and it appeared that, notwithstanding they now still transmitted considerably more light than the red glasses, they remedied the former inconvenience of an irritation arising from heat. Repeating these trials several times, I constantly found the same result; and, the sun in the first case being of a deep red colour, I surmised that the red-making rays, transmitted through red glasses, were more efficacious in raising a sensation of heat, than those which passed through green, and which caused the sun to look greenish. In consequence of this surmise, I undertook the investigations which have been delivered under the two first heads.

As soon as I was convinced that the red light of the sun ought to be intercepted, on account of the heat it occasions, and that it might also be safely set aside, since it was now proved

that pale green light excels in illumination, the method which ought to be pursued in the construction of a darkening apparatus was sufficiently pointed out; and nothing remained but to find such materials as would give us the colour of the sun, viewed in a telescope, of a pale green light, sufficiently tempered for the eye to bear its lustre.

To determine what glasses would most effectually stop the red rays, I procured some of all colours, and tried them in the following manner.

I placed a prism in the upper part of a window, and received its coloured spectrum upon a sheet of white paper. Then I intercepted the colours, just before they came to the paper, successively, by the glasses, and found the result as follows.

A deep red glass intercepted all the rays.

A paler red did the same.

From this, we ought not to conclude that red glasses will stop the red rays; but rather, that none of the sun's light, after its dispersion by the prism, remains intense enough to pass through red glasses, in sufficient quantity to be perceptible, when it comes to the paper. By looking through them directly at the sun, or even at day objects, it is sufficiently evident that they transmit chiefly red rays.

An orange glass transmitted nearly all the red, the orange, and the yellow. It intercepted some of the green; much of the blue; and very little of the indigo and violet.

A yellow glass intercepted hardly any light, of any one of the colours.

A dark green glass intercepted nearly all the red, and partly also the orange and yellow. It transmitted the green; intercepted much of the blue; but none of the indigo and violet.

A darker green glass intercepted nearly all the red; much of the orange; and a little of the yellow. It transmitted the green; stopped some of the blue; but transmitted the indigo and violet.

A blue glass intercepted much of the red and orange; some of the yellow; hardly any of the green; none of the blue, indigo, or violet.

A purple glass transmitted some of the red; a very little of the orange and yellow: it also transmitted a little of the green and blue; but more of the indigo and violet.

From these experiments we see, that dark green glasses are most efficacious for intercepting red light, and will therefore answer one of the intended purposes; but since, in viewing the sun, we have also its splendour to contend with, I proceeded to the following additional trials.

White glass, lightly smoked, apparently intercepted an equal share of all the colours; and, when the smoke was laid on thicker, it permitted none of them to pass.

Hard pitch, melted between two white glasses, intercepted much light; and, when put on sufficiently thick, transmitted none.

Many differently-coloured fluids, that were also tried, I found were not sufficiently pure to be used, when dense enough to stop light.

Now, red glasses, and the two last-mentioned resources of smoke, and pitch, any one of which, it has been seen, will stop as much light as may be required, had still a remaining trial to undergo, relating to distinctness; but this I was convinced could only be decided by actual observations of the sun.

As an easy way of smoking glasses uniformly is of some

consequence to distinct vision, it may be of service here to give the proper directions, how to proceed in the operation.

With a pair of warm pliers, take hold of the glass, and place it over a candle, at a sufficient distance not to contract smoke. When it is heated, but no more than still to permit a finger to touch the edges of it, bring down the glass, at the side of the flame, as low as the wick will permit, which must not be touched. Then, with a quick vibratory motion, agitate it in the flame from side to side; at the same time advancing and retiring it gently all the while. By this method, you may proceed to lay on smoke to any required darkness. It ought to be viewed from time to time, not only to see whether it be sufficiently dark, but whether any inequality may be perceived; for, if that should happen, it will not be proper to go on.

The smoke of sealing-wax is bad: that of pitch is worse. A wax candle gives a good smoke; but that of a tallow candle is better. As good as any I have hitherto met with, is the smoke of spermaceti oil. In using a lamp, you may also have the advantage of an even flame extended to any length.

Telescopic Experiments.

No. 1. By way of putting my theory to the trial, I used two red glasses, and found that the heat which passed through them could not be suffered a moment; but I was now also convinced that distinctness of vision is capitally injured, by the colouring matter of these glasses.

No. 2. I smoked a white glass, till it stopped light enough to

permit the eye to bear the sun. This destroyed all distinctness; and also permitted some heat to come to the eye, by transmitting chiefly red rays.

No. 3. I applied two white glasses, with pitch between them, to the telescope; and found that it made the sun appear of a scarlet colour. They transmitted some heat; and distinctness was greatly injured.

No 4. I used a very dark green glass, to stop heat; and behind it, or towards the eye, I placed a red glass, to stop light. The first glimpse I had of the sun, was accompanied with a sensation of heat; distinctness also was materially injured.

No. 5. I used a dark green and a pale red; but, the sun not being sufficiently darkened, I smoked the red glass, and, putting a small partition between the two, placed the smoke towards the green glass. This took off the exuberance of light; but did not remedy the inconvenience arising from heat.

No. 6. I used two pale green glasses; smoking that next to the eye, and placing it as in No. 5, so that the smoke might be inclosed between the two. This acted incomparably well; but, in a very short time, the heat which passed the first glass, (though not the second, for I felt no sensation of it in the eye,) disordered the smoke, by drawing it up into little blisters or stars, which let through light; and this composition, therefore, soon became useless.

No. 7. I used two dark green glasses, one of them smoked, as in No. 5. These also acted well; but became useless, for the reason assigned in No. 6, though somewhat less smoke had been required than in the former composition. I felt no heat.

No. 8. I used one pale green, with a dark green smoked glass upon it, as in No. 5. It bore an aperture of 4 inches very

well, and the smoke was not disordered; but, when all the tube was open, the pale green glass cracked in a few minutes.

No. 9. Placing now a dark green before a smoked green, I saw the sun remarkably well. In this experiment, I had made a difference in the arrangement of the apparatus. The cracking of the glasses, I supposed, might be owing to their receiving heat in the middle, while the outside remained cold; which would occasion a partial dilatation. I therefore cut them into pieces about a quarter of an inch square, and set three of them in a slider, so that I could move them behind the smoked glass, without disturbing it. After looking about three or four minutes through one of them, I moved the slider to the second, and then to the third. This kept the glasses sufficiently cool; but the disturbance of the alterations proved hurtful to vision, which requires repose; and, if perchance I stopped a little longer than the proper time, the glass cracked, with a very disagreeable explosion, that endangered the eye.

No. 10. Two dark green glasses, both smoked, that a thinner coat might be on each; but the smoke still contracted blisters, though less dense than before.

No. 11. To get rid of smoke intirely, I used two dark green glasses, two very dark green, two pale blue, and one pale green glass, together. Distinctness was wanting; nor was light sufficiently intercepted.

No. 12. A dark green and a pale blue glass, smoked. The green glass cracked.

No. 13. A pale blue and a dark green glass, smoked. The blue glass cracked. The eye felt no sensation of heat.

No. 14. Two pale blue glasses, one smoked. The first glass cracked.

It was now sufficiently evident, that no glass which stops heat, and therefore receives it, could be preserved from cracking, when exposed to the focus of pencils. This induced me to try an application of the darkening apparatus to another part of the telescope.

The place where the rays are least condensed, without interfering with the reflections of the mirrors, is immediately close to the small one. I therefore screwed an apparatus to the speculum arm, into which any glass might be placed.

No. 15. A dark green glass close to the small speculum, and smoked pale green in the focus of pencils, as before. I saw remarkably well.

No. 16. The dark green as before; but, that more light might be admitted, a white smoked glass near the eye. Better than No. 15; but the green glass cracked.

No. 17. A very dark green and white smoked glass, as before. Very distinct, but the green glass cracked in about six or seven minutes.

No. 18. A dark blue glass, as in No. 15, and white smoked. This was distinct; and no heat came to the eye. The sun appeared ruddy.

No. 19. A dark blue and a yellow glass, close together, as in No. 15, and a white smoked one, as before. This was not distinct.

No. 20. A purple glass, as in No. 15, with a white smoked one. This gave the sun of a deep orange colour, approaching to scarlet. It was not distinct.

No. 21. An orange glass, as in No. 15, with a white smoked one. The colour of the sun was too red.

No. 22. A white smoked glass, as in No. 15, without any

other at the eye. This gave the sun of a beautiful orange colour; but distinctness was totally destroyed.

No. 23. The heat near the small speculum being still too powerful for the glasses, I had a bluish dark green glass made of a proper diameter to be inclosed between the two eye-glasses of a double eye-piece. All glass I knew would stop some heat; and was therefore in hopes that the interposition of this eye-glass would temper the rays, so as in some measure to protect the coloured glass. In the usual place near the eye, I put two white glasses, with a thin coat of pitch between them. These glasses, when looked through by the natural eye, give the sun of a red colour; I therefore entertained no great hopes of their application to the telescope. They darkened the sun not sufficiently; and, when the pitch was thickened, distinctness was wanting.

No. 24. The same glass between the eye-glasses, and a dark green smoked glass at the eye. Very distinct. This arrangement is preferable to that of No. 15; after some considerable time, however, this glass also cracked.

No. 25. I placed a very dark green glass behind the second eye-glass, that it might be sheltered by both glasses, which in my double eye-piece are close together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glass receives them on a large surface, and stops light and heat, in the proportion of the squares of its diameter now used, to that on which the rays would have fallen, had it been placed in the focus of pencils. And, for the same reason, I now also placed a dark green smoked glass close upon the former, with the smoked side towards the eye, that the smoke might

likewise be protected against heat, by a passage of the rays through two surfaces of coloured glass.

This position had moreover the advantage of leaving the telescope, with its mirrors and glasses, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which must be occasioned, by the interposition of the heterogeneous colouring matter of the glasses and of the smoke.

No. 26. I placed a deep blue glass with a bluish green smoked one upon it, as in No. 25, and found the sun of a whiter colour than with the former composition. There was no disagreeable sensation of heat; though a little warmth might be felt.

No. 27. I used two black glasses, placed as in No. 25. Here there was no occasion for smoke; but the sun appeared of a bright scarlet colour, and an intolerable sensation of heat took place immediately. I rather suspect that these are very deep red glasses, though their outward appearance is black.

In order to have a more sure criterion of heat, I applied Dr. WILSON'S thermometer, No. 2, to the end of the eye-piece, where the eye is generally placed. With No. 25, it rose from 34 to 37 degrees. With No. 26, it rose from 35 to 46; and, with No. 27, it rose, very quickly, from 36 to 95 degrees. I am pretty sure it would have mounted up still higher; but, the scale extending only to 100, I was not willing to run the risk of breaking the thermometer by a longer exposure.

It remains now only to be added, that with No. 25 and 26 I have seen uncommonly well; and that, in a long series of very interesting observations upon the sun, which will soon be

communicated, the glasses have met with no accident. However, when the sun is at a considerable altitude, it will be advisable to lessen the aperture a little, in telescopes that have so much light as my 10-feet reflector; or, which will give us more distinctness, to view the sun earlier in the morning, and later in the afternoon; for, the light intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than we can soften it, by laying on more smoke upon our darkening glasses. Now, as few instruments in common use are so large as that to which this method of darkening has been adapted, we may hope that it will be of general utility in solar observations.

Slough, near Windsor,
March 8, 1800.